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ABSTRACT

Concerns raised in 1967 about the state of British research on language teaching and learning are still applicable, including a need (Birmingham, England, February 24-26); see FL 016 092. teaching, and to a lesser extent, a lack of published information on the applications of academic psychology to language teaching. Early initiatives in these areas had limited success because they failed to account for structural factors affecting the people available to do research in the field and because recommendations for research were out of touch with the broader research climate. Those mistakes can be avoided in part by (1) involving young researchers and making the field an attractive one to work in, (2) finding ways of involving teachers and other consumers of research in ongoing projects and creating a climate in which consumers can commission specific research studies, and (3) producing a list of basic research tools that need to be developed and persuading funding bodies about the value of developing them. (MSE)

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SOME PRIORITIES FOR RESEARCH IN SECOND LANGUAGE LEARNING

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2. Some priorities for research in second language learning

Paul Meara

As far as I have been able to ascertain, the last time the topic of this conference was formally discussed in a public forum was at a meeting of the Committee on Research and Development in Modern Languages (CRDML) in 1967. At that meeting, Donald Broadbent submitted a paper entitled 'Notes on current knowledge concerning the psychology of learning modern languages' (subsequently published), in which he reviewed the current state of research, and suggested areas which would profit from active development. Broadbent drew attention to the lack of any coherent programme of research on second language learning, and pointed out how little of the available research was in any sense empirical. He discussed a number of areas of research in psychology which might be able to throw light on second language acquisition. He gave some consideration to the way contemporary developments in language teaching practice only partially reflected what was becoming known about the psychology of learning. The paper concluded by suggesting three areas where Broadbent felt that useful research could be carried out: the role of meaning in learning languages; the optimum point for introducing formal grammar; and the development of perceptual discrimination in the second language. In short, several needs and a set of priorities were identified.

The paper makes depressing reading for those of us involved in L2 research in the 1980s. While the extent of our knowledge about L2 acquisition is not perhaps as parlous as it was in the late sixties, many of the criticisms that Broadbent made are still applicable, and in many areas, our understanding of language learning has made little real progress. Broadbent's three conclusions are still largely true, at least as far as research in the UK is concerned. Broadbent wrote:

> '(a) There is very little indeed that has been published directly in this area using the methods and criteria of academic psychology.
>
> (b) There is a very large amount of expertise and opinion-based activity amongst language teachers, which is based on assumptions about human behaviour and which seem to work. It would not however come up to the scientific standards of a purist psychologist.
>
> (c) There are a large number of areas of general psychology which are of relevance, but the full implications have not been

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worked out in the special situation of language learning.' (Broadbent, 1967).

It seems to me that (b) and (c) still apply without reservation. (a) is slightly less easy to evaluate, however, as there has been a considerable growth in research on L2 acquisition in the last 20 years. On the other hand, research in most fields has mushroomed over the same period, and it is probably true to say that though the absolute number of research projects in this field has grown, it has probably declined as a fraction of the total amount of research in the UK. In any case, the total number of projects actively being pursued in the area is very small. The list of ongoing research projects supplied by CILT for this conference amounts to 23 items. A number of these projects have actually lapsed, a number are only tangentially related to language learning, and of the remainder, only a handful seem to involve empirical research which is likely to be applicable in the long term. Much the same picture emerges from other surveys. Vivian Cook's (1978) survey, for example, lists just over 100 articles and publications dealing with the psycholinguistics of second language learning. Only ten of these are authored by people working in the UK, and only half of these are reports of proper empirical research work. The quality of this work is difficult to assess, but using Broadbent's 'criteria of academic psychology' would reduce the number of significant studies to a mere handful.

In this respect, then, the initiatives that the CRDML took to foster research in L2 learning appear to have been something of a failure. Research of the sort they envisaged does not seem to have materialised in response to their efforts. No coherent policy for research in L2 acquisition emerged as a result of the Committee's discussions, and no tradition of empirical work in the field was generated. Given the similarities between the concerns of the CRDML and the concerns of this conference, it is perhaps worth pondering this failure, in order to understand why the CRDML's initiatives over research were much less successful than they ought to have been.

It seems to me that it is possible to identify four principal factors in L2 research which make it different from straightforward 'academic psychology'. Each of these factors makes it difficult to undertake and organise empirical research in our field, and imposes some constraints on the type of research that can be envisaged.

Firstly, the language teaching profession is a peculiar one in that it does not naturally produce people with an orientation towards research. Few language teachers have any training in a research-based discipline. Most of us studied literature at university; some of us may have done research of a literary sort. Few of us are really numerate, however, and hardly any of us have any formal training in research methods, statistics, computing, or any of the other skills which form the stock in trade of the researcher. This

means that language teachers typically think about language learning in ways which are not easy to convert into good empirical research. Most of us will be familiar with the professor of literature who wants to find the best way of teaching French to second-year undergraduates, for example, or the teacher of German who wants to show that his new textbook produces better results than any other. It is obvious to me, and to anybody with a proper research training, that questions such as this are inappropriate topics for formal research projects. It is also clear, however, that this inappropriateness is far from obvious to many of our colleagues, and some of us may even be able to recall projects of precisely this sort which have been set up and funded in the not too distant past.

The lack of proper research training also shows itself in the staffing of university departments where applied linguistics and related studies are taught. These departments have typically tended to recruit older members of staff with long teaching experience; research experience, especially empirical research experience, has not been a high priority for these centres. The reasons for this emphasis are obvious, and indeed highly laudable, but again, it has placed considerable limitation on the type of research work on language learning that these institutions can undertake.

Another facet of the same problem is that there is no tradition in the UK of employing bright young graduate students on research projects in our field. Most of the active people in research in this country are in their late thirties, and many are considerably older than this. This situation contrasts dramatically with other subjects, where much of the innovative, state-of-the-art research is produced by young graduate students in their early twenties. We have no obvious career structure for young people interested in pursuing research into L2 acquisition, and consequently, few bright, properly trained young people ever think of doing research in this area. (See Christopher Brumfit's 'Schematic representation' on p 76).

These demographic and structural considerations have had a marked effect on the type of research that is carried out in the UK. Innovative and speculative we may be, but empirical we are not.

A second factor which the CRDML perhaps underestimated is that empirical research costs money, and requires a basic level of facilities which many departments just do not have. Many colleagues working on language learning belong to language departments, which traditionally do not have research budgets. Even departments of linguistics and language centres tend to be grossly underequipped and underfinanced, compared with what one would expect in, say, a typical psychology department. In addition, until very recently, the SSRC/ESRC and other funding bodies have appeared to be very reluctant to fund research projects in second language acquisition. The result of this has been that those of us who have attempted to do

empirical research work have been hampered by lack of equipment and resources. This can be seen in the studies in CILT's list, most of which are small-scale projects, with no visible means of support, using small numbers of subjects, and a pencil and paper technology. In these circumstances, it is hardly surprising that good people who might have been interested in the area have been seduced away to other areas where money, equipment and tangible rewards have generally been more freely available.

Another factor which makes research into language learning rather different from other similar areas is that there is no real tradition of co-operative research in our field. In most sectors of the economy there is a tradition of close co-operation between university departments and the consumers of basic research - chiefly industry and government - and it is common for external agencies to commission research in important areas, or to indicate problem areas for which urgent solutions are required. In our field, there are very few examples of collaborative research of this kind. With the exception of the National Foundation for Educational Research, perhaps, most of the research we do tends to be rather theoretical in nature, and rather unresponsive to demands made by consumers in as much as they are articulated at all. Certainly, the dominant model of research in the UK is a hierarchical one, where expert researchers produce ideas and pass them on to language teachers whose job is to implement them as they see fit. There is, however, very little traffic in the other direction. It is very difficult for teachers' groups to commission research, or even to indicate what sorts of research might be useful to them. There are hardly any instances of publishers sponsoring research work, though the recent establishment of a lexicography research unit is an interesting example of what publishers might do. The private sector in English language teaching is not involved in research to any serious extent either, despite the fact that the size of the EFL operation in this country makes it a major contributor to the economy.

This situation seems to me to be an unfortunate one. It means that the research we do is often reduced to an intellectual exercise, with no natural audience. As a result, a lot of this work, even when it is good quality research, is likely to go unread, and never reach the people who could make best use of it. More seriously, it means that an unavoidable split develops between those of us who see ourselves primarily as language teachers and those of us who think we are primarily researchers - a split that is amply in evidence at this conference.

None of these factors is a particularly recent phenomenon. All of them existed in the late sixties, just as they exist now, and all of them contributed in some form or other to the poor response which the work of the CRDML produced. They are equally likely to stymie the efforts of this conference, unless we are careful to take them fully into account.

However, there was a fourth factor which contributed to the general lack of interest in the CRDML's deliberations. The Committee itself decided that it did not wish to commission research, partly, one supposes, for financial reasons, but not entirely so. The minutes of the Sub-Committee where Broadbent's paper was discussed state:

> 'It was agreed that this record of the discussion, together with Dr Broadbent's paper, should then ... be sent through CILT to Departments of Psychology at Universities and others; at the same time inviting information of relevant work being done or contemplated. Whilst avoiding 'commissioning research', to which there were strong objections, it was believed that this action might most effectively stimulate basic research relevant to the theme of the psychology of learning modern languages, and particularly to the gaps mentioned in Dr Broadbent's paper, leading to the submission of projects suitable for the Committee's consideration.' (sic)

The minutes do not record what these 'strong objections' were - though it is not hard to guess. Nor is it hard to imagine what sort of response emerged from the departments which received the paper. With hindsight, it might have been more effective for the Committee to commission some very specific research projects, and to provide some sort of financial incentives for people to work in the areas considered important. In fairness, it should be pointed out that the Committee itself was probably not well placed to carry out an initiative of this sort. Of the eight senior academics who took part in the discussions, none has consequently carried out research of the sort that Broadbent outlined, and indeed, to my knowledge only one has shown a sustained interest in the wider problems of second language learning. Add to this the limited financial power that the Committee had, and the structural problems I have already mentioned, and it is easy to see that the ground which CRDML was trying to sow was very stony indeed.

What lessons can we learn from the experience of CRDML? It seems to me that if this conference hopes to have any lasting impact on the type of research into L2 learning that is carried out in this country, then it needs to address itself to three problems.

The first problem is the isolation of researchers from the rest of the language learning and teaching community, and the élitism that this breeds. I can see two ways in which this problem could be tackled. One is to deliberately create situations where teachers and researchers can talk to each other, and generally to increase the flow of information from teachers to researchers. At the moment, for example, CILT does a splendid job disseminating and documenting research findings. It would be equally useful if someone like CILT kept a register of people with problems that needed to be solved, or even better, could act as a sort of broker, putting problems in

touch with potential solvers. If the experience of the Dutch 'science shops' is anything to go by, this sort of exchange system can be interesting, highly innovative, and produce worthwhile results on both the practical level and the theoretical one.

The second solution to the élitism problem is to involve more people in the research process. Most research in the social sciences is carried out by teams of people, with very different skills. In contrast, a large part of the research we do is carried out by one researcher, possibly with the help of a research assistant, and very little involvement on the part of anyone else, except as guinea pigs or subjects in experiments. The limitations on this type of research are obvious.

The second problem concerns personnel. At the time of the CRDML there were very few people with formal training in research methods. Fortunately, this situation is changing, and many MA courses now have some formal research training as part of their programme. Ironically, this doesn't seem to have led to a marked increase in the number of trained people in this country who hold positions of responsibility and influence, since the majority of people who take MA courses eventually go overseas. On the other hand, courses in linguistics and psychology, while not common, are less rare than they were twenty years ago, and this means that there are a number of young graduates with training in the appropriate disciplines, and often themselves bilingual, who could carry out good quality research if they were attracted into this area. The problem for us at the moment is that other areas are much more attractive to good graduate students - given a choice between a job or a prestigious Alvey project, and an uncertain position on a second language learning project, it would take a very special kind of person to opt for the latter. It seems to me that this conference ought to consider how we can generate a number of research posts with properly equipped projects and good facilities which would be an attractive proposition to young researchers.

This problem brings us back to the third problem, finance. There seems to be a general view that it is very difficult to raise money for research into second language acquisition. My own view is that the situation is not really as bad as it is sometimes made out to be, but that efforts to raise money are hampered by some very simple obstacles, which a conference of this sort ought to be able to do something about. These obstacles fall into two classes.

Firstly, there is no general feeling among the wider research community that second language learning is a problem worth investigating. This means that when I compete against other people asking for money, I am up against a credibility problem. Language and the Aged, for example, or Language in Schizophrenia, seem superficially appealing in a way that second language learning is not. This is

largely a question of public relations, and it would help very greatly if this conference could create a climate in which second language acquisition was not looked on as a third-rate research area.

Secondly, I have found that it is often difficult to develop a large-scale research proposal because many of the basic research tools that I would need in a large project have not been developed. For example, if I want to do a study of vocabulary development in advanced adult learners of Spanish, then I need some accurate and reliable measure of how advanced my subjects are, some measure of how well individual vocabulary items have been internalised, and probably other ancillary measures such as attitude profiles and measures of what might broadly be called language learning style. None of these measures currently exists, and so when I frame my grant application, I am severely constrained. Either I have to develop these tests myself from scratch, which increases the length of the project, and pushes up its cost, or I ignore the problem, and leave myself open to the charge of being naïve. Either way, my application compares poorly with what, say, a psycholinguist might be able to achieve with the same amount of money. At the same time, of course, applications for small grants to develop tools of this sort do not seem to meet with favour either, perhaps because they seem essentially trivial, or perhaps because the need for them is not apparent to those working in other fields. It would be extremely useful if this conference could identify a list of basic research tools that need to be developed, and if we could produce a convincing case that might persuade the ESRC and other bodies that it would be worth commissioning them.

To sum up then, in this paper, I have used the experience of the CRDML as a convenient handle on which to hang a number of ideas that I would like this conference to consider. It seems to me that the CRDML initiatives were less successful than they might have been because they did not take into account a number of structural factors that affect the people available to do research in our field. They also failed because the recommendations of the CRDML were limited to exhortations which were out of touch with the wider research climate. I hope that this conference will be able to avoid this set of mistakes, and especially that it will be able to make recommendations in three specific areas: a) to find ways of involving young research workers in research projects, and making the field an attractive one to work in; b) to find ways of involving teachers and other consumers of research in ongoing research projects, and to create a climate in which consumers can commission specific research studies; c) to produce a list of basic research tools which need to be developed, and to persuade funding bodies of the value of developing them.

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